compared with facts, the terms to be used are "hypothetical" and "practical."

This is one of three advertised books of "The Specialists' Series" which deal with electric engineering. Another of the three is devoted to the subject of magneto- and dynamo-electric machines, and the third is on electric lighting. We think it probable that in the greater part of Mr. Kapp's book he is going over ground which belongs almost altogether to the author of one of the other books of the series. As Mr. Kapp treats his subjects well, however, we cannot much object to this; but what we do object to is, that while taking up the subjects of the other authors, he has not given us his own subject. In sixty-three pages, or about one-fifth of the book, an instructive account is given of the various attempts which have been made to drive carriages on railways, telpher lines, ploughs, cranes, fans, and pumps, and we understand from Mr. Kapp's introduction that it is to this sort of transmission of energy that his book is devoted.

Now it is not merely sufficient for the author to give an account of what has already been done in this way; the reader expects a correct theoretical treatment of the whole subject, the cost of conductors, the fall of potential along the conductor, and the efficiency of transmission. These questions are sufficiently well taken up for a treatise on electric lighting, but for a book on the electric driving of machines at a distance the subject can hardly be said to be touched upon. Thus, for example, the development of Sir William Thomson's law as applied in electric light installations, and published by Prof. Forbes in his lectures at the Society of Arts, is carefully given. Now small alteration of potential difference at an incandescent lamp may produce disastrous effects on the lamp, may destroy it, or may cause sudden darkness, and this is the most important consideration in arranging conductors for lighting purposes; whereas, in the electric driving of trains or machinery, small alterations of potential difference are of no importance whatsoever. In consequence of this, in driving machinery electrically there may be a very considerable fall of potential along the conductor from the dynamo to the motor, and hence motors may be worked directly at distances which it would be absurd to contemplate in working an incandescent lamp. In fact the question of cost of conductors must be treated from quite a different point of view in the two cases, and it seems to us that Mr. Kapp has taken up the point of view which is most remote from his subject.

We think Mr. Kapp's book a very valuable addition to electrical engineering literature. It will be widely read, and it deserves the popularity which it will receive. Had we not thought it to be so excellent in many ways we should not have criticised it so narrowly, and, in spite of our warning to the student, we are very glad to meet with originality in leading up to the theory of the dynamo-machine. We are glad to see that the author has slightly amplified his account of the method, now in general use, of calculating the probable electromotive force of a dynamo-machine, which he published in his paper. The method is known to be practically correct, although it is based on a magnetic hypothesis of which there is no recognition in any book on physics—the hypothesis of magnetic resistance. We could have wished that Mr. Kapp had dwelt more upon this hypothesis, as we know of no actual results of experiment having yet been published which give it a general verification.

In reading over this criticism we feel that our objections to the book have all been brought very prominently forward. It would be very easy to point out here much that is good in the book, but perhaps our readers would then find this article long and tedious. Any reader of the book will find original and interesting views in every chapter; it is not every reader who would for himself have noticed the faults which we have here gathered together. We have achieved the difficult task of finding fault with an excellent book.

JOHN PERRY

OUR BOOK SHELF

The Aryan Maori. By Edward Tregear. (Wellington, N.Z.: George Didsbury, 1885.)

THIS little book contains a theory that the ancestors of the New Zealanders belonged to the Aryan race, and were a pastoral people. To signify this, the cover is adorned with a golden picture, seemingly representing a Maori warrior in native guise, accompanied by a sturdy little Highland bull. Now, it being notorious that the New Zealanders, when discovered, had no cattle nor remains of them in their country, the reader's curiosity is aroused to see how Mr. Edward Tregear supports this unlikely thesis. His method proves to be a philological paradox which we have never met with before. For example, it is argued (p. 31) that the Maoris once knew the bull by a word like the latin *taurus*, a bull. How so, one asks, when they no more had the word in their vocabulary than the beast on their land? The answer is, that in the absence of the word *taurus* itself the author relies on a dozen or so of other Maori words which he alleges to refer to it. The following are a few of them:—*Tara*, had courage; *tararau*, made a loud noise; *tararua*, had two points or peaks; *tareha*, was red; *tarehu*, caught one unawares; *tarore*, had a noose put on him; *taruke*, lay dead in numbers (if it was characteristic of the bulls to lie dead in numbers, how multitudinous the cows and calves must have been in the Aryan-Maori herds!). The poverty of the Maori language in consonants makes it easy to the author to play this fanciful game with his dictionary to his own full satisfaction. He takes a real interest in studying the Maoris, and though he has gone astray this time, he may, if a young man, do something more worth doing in the collection of native customs, legends, games, and the like which the older natives still remember.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Tidal Friction and the Evolution of a Satellite

IN NATURE, vol. xxxiii. p. 367, is an article by Mr. G. H. Darwin, defending his theory of tidal evolution, and dealing with what I have written respecting that theory. Space will here prevent my replying at length to the above; but as the author of it seems to think that my inquiry has been confined too much to the mode of origin of the moon, I have pushed it out in other directions, when important results have been obtained. I purpose here chiefly devoting my space to these, which can be put in a comparatively short and simple form.

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But before entering on the new ground I think a few words of explanation will be necessary. Mr. Darwin takes exception to a proposition of mine in that it holds his theory to be dependent upon the genesis of the moon at the present surface of the earth. I was led to this conclusion chiefly from the apparent stress laid on the condition by the writer on that subject in Thomson and Tait's "Natural Philosophy," but as the author now states that his theory is not so founded, I think it ought to be allowed that it is not. But I think the argument can be put in another way; for if the moon be allowed to have separated at a period over four hours, the theory would be at variance with the calculations of its author (for he fixes the period at between two and four hours). A flock of meteorites is proposed as a form in which the moon might have receded from the earth. Nothing can be gained by this, for the flock of meteorites cannot come nearly so close to the earth as the moon in a single mass, without the constituent members being separated and each compelled to describe an independent orbit with its own period. In other words, the tidal force would separate the flock of meteorites at a greater distance than it would the single body. And at a greater distance it is not necessary, as the moon in her conglomerate form could not be objected to.

Also, he quotes and questions a passage, which is to the effect that two heavenly bodies cannot revolve about their centre of inertia with their surfaces nearly in contact, unless one is smaller than, and denser than, the other by a certain amount. The case was intended for where the two bodies move as parts of a rigid body, *i.e.* each keeps the same face towards the other; but I omitted to insert this condition in giving the rule.

Coming now to the results of my second investigation. In his last reply, as well as in several other places, Mr. Darwin advances the Martian system as affording a striking confirmation of the influence of tidal friction. The view is that the extreme minuteness of the inner satellite has preserved it as a standing memorial of the primitive time of rotation of Mars round his axis (see the *Observatory*, July, 1879).

Now I think it must have been taken for granted that the smallness of the satellite would allow the above state of things to come about, for an estimate of the comparative effects produced on Mars by solar and by satellite tidal friction, and the reaction on the satellite shows that, according to the estimated dimensions of the latter body, its period must be considerably more disturbed than that of Mars. Prof. Newcomb estimates the diameter of the inner satellite at from 10 to 40 miles. If we take the lower estimate and suppose the body to be only as dense as the sun, then its mass will be 86000^3 times smaller than the sun's. But the distance of the satellite—6000 miles—is 23333 times less than the sun's distance, and this number must be cubed to get the effect on the tidal force through greater proximity. After making the proper allowances, as above, it will be found that the satellite has a tidal force fifty times less than that of the sun. If the tidal retardation or acceleration, as the case may be, varies as the square of the tidal force, as Mr. Darwin allows, then 1/2500 of the planet's retardation must be counteracted by the satellite tides, which go round in the reverse direction to the solar tide. Here the reaction on the satellite must be considerable, for an approximate calculation will show that the orbital momentum of that body is only about 1/2,800,000 of the planet's rotational momentum. It will not be necessary to work out the calculation. Suffice it to say that the density and dimensions of the planet are taken from Newcomb's tables, and that the distribution of density is supposed to be like that of the earth, giving a rotational momentum equal to that of homogeneous density multiplied by 0·83.

If solar tides lengthen the Martian day by one minute, then the rotational momentum will have been reduced by about 1/1400 of the whole, and the satellite must have produced an effect in the opposite direction 2500 times as small, so that the actual effect of the satellite is to increase the rotational momentum by 1/3,500,000. And since the reaction on the satellite will be equal and in the opposite direction, more than half its momentum (which is 1/2,800,000 of the planet's) will be lost, which will reduce its distance to the surface of the planet. Hence we are led to the startling conclusion that before solar tidal friction can alter the rotation-period of Mars by one minute, the inner satellite must fall into the planet. I have not taken into the calculation the circumstance that the satellite tide goes round quicker than the solar one, nor that, as the satellite approached the planet, its tides would increase, the purpose here being only to give an account of the relative changes that should take place at about the present configuration. Further, it would seem that solar tides could not have reduced the period of Mars much, even if it be supposed that the inner satellite was first at any greater distance, for then it must either have gone out and attained a longer period than Mars, or it must have fallen into the central body long ere this. There seems no escape from these conclusions, unless the little body gives out an extraordinary amount of light for its size (for its probable size is judged by its brightness), but this seems so improbable, that it would be unreasonable to suppose so. As for the density, the inferior limit must be not less than half that allowed, otherwise the tidal force of the planet would break the body up.

Now a few words may be said on the future history of the moon. Before, I have said that the tracing back of the moon has apparently been carried too far in one direction; and now I think that the tracing forward, supposing tidal friction to have free play in the future, has been carried too far in the other direction. According to Thomson and Tait ("Elements of Natural Philosophy"), the moon's distance should increase to 347,100 miles by the time the earth's rotation relatively to the moon is stopped, when the two bodies should revolve in about 48·3 days. Now it would take all the rotative momentum that the earth would lose during the change to send the moon to the above distance; or, in other words, if there were no solar tides or other causes to prevent all the rotational momentum lost by the earth going to increase the moon's orbital momentum, the moon's distance would not be increased to beyond the above. But it is clear that a considerable portion of the rotative energy would be lost in solar tidal friction, which would have no part in increasing the moon's distance from the earth. For the moon to recede to the distance named, the earth must not only have its present moment of momentum, but also as much as solar tides would extract during the interval. At the present time the retardation through solar tides is not a small fraction of the whole, and it should increase till, at the other end of the journey, it will be more than half the whole retardation, for then the solar will be greater than the lunar tide.

I believe that Messrs. Darwin and Ball, who wrote a year or two before the date of Thomson and Tait's work, give the distance to which the moon will recede as even greater than the above, and say her period of revolution will be about fifty days. Certain remarks made by Mr. Darwin in the larger work of Thomson and Tait leave some doubt as to whether a correction has recently been made in the moment of momentum of the earth's rotation, but even if the earth be supposed homogeneous, the rotational momentum would not be sufficient to send the moon to the above distance, when allowance is made for solar tidal retardation. Hence it seems that no allowance has been made for the effects of this agency, and that when such allowance is made, the moon's destination must fall far short of the estimates given. If the distance (347,100 miles) be reduced to about 320,000 miles, I think it would be nearer the mark.

Mr. Darwin says that the eccentricity of the lunar orbit, the obliquity of the ecliptic, and other elements would be coordinated together by supposing that the moon first had a separate existence at no great distance from the present surface of the earth, and with small differential motion with respect thereto. I will only say that the case is so complicated, and the data so unreliable, that the results of the calculations involved seem to be little better than guess-work.

As for the distribution of satellites in the solar system, I think the majority of diverse theories would hold that they should be more numerous far from the sun, for the simple reason that solar disturbance would be less there. JAMES NOLAN

Dergholm, Victoria, May 25

MR. NOLAN is correct in supposing that I made no numerical calculation with regard to the inner satellites of Mars. I accept the calculation which he gives, and admit that the present period of revolution of the satellite cannot be regarded, as I supposed, as a memorial of the primitive period of rotation of the planet.

I see, however, no reason, as yet, to recede from the following statement (*Phil. Trans.*, Part II. 1880, p. 883), which embodies the essence of the argument, without the erroneous phrase:—

"It is here (in the case of Mars) alone in the whole system that we find a satellite moving orbitally faster than the planet rotates. This will also be the ultimate fate of our moon, because, after the moon's orbital motion has been reduced to

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